globules of sulphurous acid and liquid carbonic acid while in a "spheroidal condition." In these cases, notwithstanding the proximity of the hot vessel, the temperatures of the globules of SO_2 and CO_2 are respectively as low as $-10°$ and $-73°$ C.

It has long been remarked by physicists that some substances pass *directly* from the *solid* to the *gaseous* state, without undergoing liquefaction: that is, when heated, *they sublime without melting*. Such bodies, under ordinary atmospheric pressures, have their *boiling* points *lower* than their temperatures of *fusion*; hence they *volatilise* without *melting*. Moreover it has long been known that such substances may be made to *fuse* by subjecting them to an *abnormal pressure* sufficient to *raise* their boiling points *above* their points of fusion. Thus the classical experiments of Sir James Hall show that carbonate of lime may be *fused* when heated under a pressure sufficient to prevent the CO_2 from escaping (*Trans.* Royal Soc. Edin., vol. vi. pp. 71-186, 1805). In like manner metallic arsenic sublimes without melting at 180° C., under the ordinary pressure of the atmosphere; but the experiments of Landolt in 1859 show that under artificial pressure it melts in globules at a low red heat. It is evident that in these cases the *rapid vaporisation* of the solids under ordinary circumstances *prevents* the temperature from reaching the point of fusion; but when subjected to additional pressure the conditions of liquefaction are secured. On the other hand, in the case of *ice*, it is obvious that the *withdrawal of pressure* by *lowering* its boiling-point places it in the same category with metallic arsenic under ordinary conditions of pressure.

In relation to the *literature* of this subject it is proper to add the following quotations from M. V. Regnault's "Elements of Chemistry" (American Translation, Philadadelphia, 1865, vol. i. p.279). In speaking of the fusion of metallic arsenic under pressure he says:—"The distance between the point of fusion and that of ebullition of any body may, however, be increased at pleasure. For *the point of ebullition of a body is the temperature at which the tension of its vapour is equal to the pressure exerted upon it*; and hence by increasing the pressure the boiling-point is raised without sensibly affecting the point of fusion." Again, he says:—"Reciprocally it is evident that a *volatile solid body* may be always subjected to so slight a pressure that it will *boil* at a temperature *inferior* to that at which it *melts*. Thus ice at the temperature of $-1°$ C. possesses an elastic force represented by 4·27 mm.; in other words, it boils at a temperature of $-1°$ C. under the pressure of 4·27 mm. Ice may therefore be entirely volatilised *by ebullition* under this feeble pressure, without reaching its point of fusion, which is 0° C."

Berkeley, California, September 30 JOHN LECONTE

Wire Torsion

THE phenomena described by Major Herschel in his letter to NATURE, vol. xxii. p. 557, and about which he asks for information, are, we think, quite easily explained by what is known of the fluidity of metals. Yielding, or flowing, seems to occur in all metals after a certain limiting stress has been reached; indeed it probably occurs, although perhaps to an immeasurably small extent, even with small stresses (see *Proc.* Roy. Soc. No. 204, p. 411, 1880); but there is generally a limiting stress beyond which the increase of strain due to yielding becomes comparable in magnitude with the ordinary strains, which instantaneously disappear on the removal of the load. The bell-smith pulls his copper wire, and makes it much longer before he thinks it fit for use; in a similar way the telegraph constructor stretches, or *kills* the iron wire before he erects the line. Up to a certain limit of pulling force, the wire obeys the well-known laws of elasticity; slightly above that limit there is considerable fluid-yielding, there being but very little yielding below that limit; and at any instant during the lengthening if the man ceases to pull, the wire shortens a little. In fact at any stage the wire obeys the elastic law for *small* stresses. Eventually the man ceases to pull, knowing that the metal has lost most of its fluid properties, which can only be restored to it by annealing. The same thing occurs in brass, although to a smaller extent than in copper, which can be experimentally proved in the following way:—Stretch a piece of well-annealed brass wire in the manner described by Major Herschel until it is nearly breaking; and immediately set the wire vibrating. Now the note given out by the stretched brass wire, which, as is well known, depends on the tensile stress, will be found rapidly to go down in pitch. If the wire be tightened up again sufficiently with the screw, the original note will again be heard, and the pitch will again go down, but not so rapidly as before. Repeat this process until no flattening of the note is heard; then in this state we think that the experimenter will find the wire to break with much less torsion than before, and to obey Hooke's law more exactly. If it be desired to repeat the yielding or flowing process, the wire must be previously again annealed.

Mere sudden straining, even nearly up to the breaking stress, is not sufficient to destroy the fluidity of brass; time is required. The yielding behaviour of a brass beam when loaded has been studied by Prof. Thurston (*Trans.* American Soc. of Civil Eng. vol. vi. p. 28), and we may add that we have found that the permanent state is always more rapidly reached when the wire is subjected to rapid vibrations.

It may be because torsion of a wire is more visible than longitudinal strains (the twist being inversely proportional to the fourth power of the diameter for a given twisting moment, whereas the longitudinal strain for a given load is inversely proportional to the square of the diameter) that fluidity is so much more apparent in torsional experiments; but we think it probable that fluidity will be found always much more apparent when the volume of the material acted on is unchanged, that is, when the stress is mainly one of shear as it is in torsion.

However this may be we can explain why wire which has been "killed" for pulling forces is not "killed" for twisting, and why it is more difficult to kill for twisting than for tensile stresses. It is well known to wire-drawers that in whatever state copper or brass wire may be, whether annealed or not, it may be drawn smaller, although no doubt it requires less care to draw it if it is annealed. We cannot merely pull wire much smaller, it has to undergo a lateral pressure such as the die gives it. Now in twisting a wire it everywhere receives this lateral pressure, that is—imagine a right-handed spiral filament being lengthened by the twist—then the other component of the twist gives to the filament a compression at right angles to its length which enables it to extend. It seems that this lateral pressure is needed to overcome some sort of friction in the particles of the metal tending to prevent their moving into the axis of the wire, and which therefore is greater as the section of the wire is larger, and it is probably for this reason that a very thin wire extends much more, for a given initial length, before it is killed than a thick wire. We have known a length of about fifteen inches of fine copper wire which had just been drawn, and which had been well killed, to bear six or seven hundred complete turns in a lathe, one end being fixed, the other end turned, and the wire kept pretty taut before it was accidentally broken, and even afterwards parts of the wire could be considerably lengthened by pulling. The nature of the explanation of this apparent annealing for tensile stresses arising from previous torsion will be gathered from what follows.

We infer that the three or four turns given to the wire at the beginning in Major Herschel's experiment were not sufficient to produce permanent torsional set; why then should increasing the tension during the torsion cause torsional set as well as lengthening of the wire? This is, we think, a more important question than the one presented to us by the observations of fluidity in the latter half of Major Herschel's letter, and which arose from the metal having belonged to what Prof. Thurston calls the "tin class" as distinguished from metals of the "iron class."

The explanation we think is as follows, and it leads to the conclusion that torsional fluidity is not independent of tensile stress:—

Suppose right- and left-handed spirals had been imagined in the wire in question, making everywhere angles of 45° with the axis of the wire; then torsional strain, however set up, would consist in the production of a difference in length of these two sets of spirals. Now a twisting moment produces this effect; it lengthens, say the right-hand spiral and shortens the left, and we know that up to a certain limit, which is tolerably high, the same effect is produced whatever be the tensile stress in the wire, which latter simply tends to lengthen both spirals equally. In fact if Hooke's law is true, the torsion is independent of the tension. But above a certain limit of pull in the wire, the strain in the direction of the right-handed spiral being everywhere due to the sum of two tensile stresses, becomes so great that fluidity sets in and permanent set is produced; whereas in the direction of the left-handed spiral the stress is due to the difference between the tensile stress and the compressive part of the torsional shearing stress, and this difference being small, no permanent tensile set is produced, or at all events one much less than in the case of the other spiral. Consequently if all stresses now cease to act

a permanent difference would remain in the lengths of the two spirals, that is, there would now be a permanent twist.

Information regarding the fluidity of tempered steel, copper, brass, lead, tin, &c., will be found in the papers of M. Tresca, and in the second of the Cantor lectures delivered by Mr. Anderson before the Society of Arts April 19, 1869, as well as in Mr. Anderson's book on the "Strength of Materials," and in Mr. Bottomley's reports communicated at the Meetings of the British Association in 1879-80. We do not think, however, that much of the valuable information on the fluidity of metals which is scattered through the *Proceedings* of the different societies has yet been collated. Wire-drawers, watch and clockmakers, as well as the makers of philosophical instruments and of other small machinery, have a considerable amount of knowledge of this subject which they cannot systematise and make known to others, but which, nevertheless, they make ready use of in their work.

Finally, we would suggest that if Major Herschel wants his wire to obey Hooke's law for small twists only, he will not find it necessary to destroy the properties which are due to its being annealed. If, however, he desires to use greater twists, it will be necessary to leave the wire under a fairly large pull for a considerable time without twisting it until it ceases to continuously yield to tensile stresses of greater intensity than that of the shear stress to which it has afterwards to be subjected. And if in Mr. Allan Broun's gravimeter it be necessary to employ such large twisting couples as Major Herschel was using in his experiments, we would suggest the employment of a longer and thicker wire.

JOHN PERRY
W. E. AYRTON

London, October 18

On the Skin-furrows of the Hand

IN looking over some specimens of "prehistoric" pottery found in Japan I was led, about a year ago, to give some attention to the character of certain finger-marks which had been made on them while the clay was still soft. Unfortunately all of those which happened to come into my possession were too vague and ill-defined to be of much use, but a comparison of such finger-tip impressions made in recent pottery led me to observe the characters of the skin-furrows in human fingers generally. From these I passed to the study of the finger-tips of monkeys, and found at once that they presented very close analogies to those of human beings. I have here few opportunities of prosecuting the latter study to much advantage, but hope to present such results as I may attain in another letter. Meanwhile I would venture to suggest to others more favourably situated the careful study of the lemurs, &c., in this connection, as an additional means of throwing light on their interesting genetic relations.

A large number of nature-prints have been taken by me from the fingers of people in Japan, and I am at present collecting others from different nationalities, which I hope may aid students of ethnology in classification. Some few interesting points may here be mentioned by way of introduction.

Some individuals show quite a *symmetrical* development of these furrows. In these cases all the fingers of one hand have a similar arrangement of lines, while the pattern is simply reversed on the other hand. A Gibraltar monkey (*Macacus innus*) examined by me had this arrangement. A slight majority of the few Europeans I have been able to examine here have it also.

An ordinary botanical lens is of great service in bringing out these minor peculiarities. Where the loops occur the innermost lines may simply break off and end abruptly; they may end in self-returning loops, or, again, they may go on without breaks after turning round upon themselves. Some lines also join or branch like junctions in a railway map. All these varieties, however, may be compatible with the general impression of symmetry that the two hands give us when printed from.

In a Japanese man the lines on both thumbs form similar spiral whorls; those of the left fore-finger form a peculiar oval whorl, while those of the right corresponding finger form an open loop having a direction quite opposite to that of the right fore-finger in the previous example. A similar whorl is found on both middle fingers instead of a symmetrically reversed whorl. The right ring-finger again has an oval whorl, but the corresponding left finger shows an open loop.

The lines at the ulno-palmar margin of this particular Japanese are of the parallel sort in both hands, and are quite symmetrical, thus differing from the Englishman's considerably. These instances are not intended to stand for typical patterns of the two peoples, but simply as illustrations of the kind of facts to be observed. My method of observation was at first simply to examine fingers closely, to sketch the general trend of the curves as accurately as possible, recording nationality, sex, colour of eyes and hair, and securing a specimen of the latter. I passed from this to "nature-printing," as ferns are often copied.

A common slate or smooth board of any kind, or a sheet of tin, spread over very thinly and evenly with printer's ink, is all that is required. The parts of which impressions are desired are pressed down steadily and softly, and then are transferred to slightly damp paper. I have succeeded in making very delicate impressions on glass. They are somewhat faint indeed, but would be useful for demonstrations, as details are very well shown, even down to the minute pores. By using different colours of ink useful comparisons could be made of two patterns by superposition. These might be shown by magic lantern. I have had prepared a number of outline hands with blank forms for entering such particulars of each case as may be wanted, and attach a specimen of hair for microscopic examination. Each finger-tip may best be done singly, and people are uncommonly willing to submit to the process. A little *hot* water and soap remove the ink. Benzine is still more effective. The dominancy of heredity through these infinite varieties is sometimes very striking. I have found unique patterns in a parent repeated with marvellous accuracy in his child. Negative results, however, might prove nothing in regard to parentage, a caution which it is important to make.

I am sanguine that the careful study of these patterns may be useful in several ways.

1. We may perhaps be able to extend to other animals the analogies found by me to exist in the monkeys.

2. These analogies may admit of further analysis, and may assist, when better understood, in ethnological classifications.

3. If so, those which are found in ancient pottery may become of immense historical importance.

4. The fingers of mummies, by special preparation, may yield results for comparison. I am very doubtful, however, of this.

5. When bloody finger-marks or impressions on clay, glass, &c., exist, they may lead to the scientific identification of criminals. Already I have had experience in two such cases, and found useful evidence from these marks. In one case greasy finger-marks revealed who had been drinking some rectified spirit. The pattern was unique, and fortunately I had previously obtained a copy of it. They agreed with microscopic fidelity. In another case sooty finger-marks of a person climbing a white wall were of great use as negative evidence. Other cases might occur in medico-legal investigations, as when the hands only of some mutilated victim were found. If previously known they would be much more precise in value than the standard *mole* of the penny novelists. If unknown previously, heredity might enable an expert to determine the relatives with considerable probability in many cases, and with absolute precision in some. Such a case as that of the Claimant even might not be beyond the range of this principle. There might be a recognisable Tichborne type, and there might be an Orton type, to one or other of which experts might relate the case. Absolute identity would prove descent in some circumstances.

I have heard, since coming to these general conclusions by original and patient experiment, that the Chinese criminals from early times have been made to give the impressions of their fingers, just as we make ours yield their photographs. I have not yet, however, succeeded in getting any precise or authenticated facts on that point. That the Egyptians caused their criminals to seal their confessions with their thumb-nails, just as the Japanese do now, a recent discovery proves. This is however quite a different matter, and it is curious to observe that in our country servant-girls used to stamp their sealed letters in the same way. There can be no doubt as to the advantage of having, besides their photographs, a nature-copy of the for-ever-unchangeable finger-furrows of important criminals. It need not surprise us to find that the Chinese have been before us in this as in other matters. I shall be glad to find that it is really so, as it would only serve to confirm the utility of the method, and the facts which may thus have been accumulated would be a rich anthropological mine for patient observers.

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[Some very interesting examples of nature-printed finger-tips accompanied this letter.—ED.]